RESPONSE TO REVIEWER 2

Major comments:

These major comments are also addressed through the response to your other minor comments, below.

There are many statements that require more discussion and clarification. In some places strong statements are made that not supported by the results of the model simulations. These statements need either be toned down, or sufficient evidence should be provided (which is probably not possible without additional experiments, especially when reference to VP models is made.)

We stress the point that no direct comparison was made between the present Maxwell-EB and VP/EVP model simulations. This is further discussed in the responses to minor comments.

For instance, the impact of the simplified model equations, geometry, wind forcing and no thermodynamics is discussed too little, especially when comparing to observations made outside of a channel. The model set-up for the analysis of PDFs of ice thickness facilitates ridging with a high amount of coastal boundaries, low cohesion parameter and strong wind forcing (and a presumably wrong source term in Equation (7)), which is not further discussed.

We have addressed issues with the thickness distribution in the revised version of the paper. These issues are discussed in the responses below. The PDFs of ice thickness produced are not the result of a "low cohesion" (see our response to your comments about cohesion below : there is no physical or observational basis to claim that the value of cohesion used for these particular simulation are "low"), nor of the amount of coastal boundaries (as idealized simulations have also been used). The strong wind forcing used here has the effect of redistributing the ice thickness starting from a uniform ice cover in a shorter amount of time. A lower wind forcing gives similar results over a longer time.

In the experiments with reduced mechanical strength a comparison to observations would be even possible: Are a collapse of ice bridges and no flow storage observed in the recent years?

As mentioned in the paper, the absence of stable ice bridges in Nares Strait was indeed observed in the year (September to August) 2006/2007. For that year, *Kwok et al., 2010* estimated an annual ice areal and volume flux equivalent to twice their average value over the 1997 to 2009 period. For the same year, *Ryan et al., 2017* observed the maximum of the median ice draft (measured across Kennedy channel, upstream Kane Basin) over the 2003 to 2012 period. *Munchow et al., 2016* reported that the ice arch between Kane Basin and Smith Sound failed to form in the winters 2006/07, 2007/08, and 2009/10, and collapsed after less than 2 months in 2008/09. They estimated an increase of 45% of the volume flux, of 69% of the ocean freshwater flux and of 46% of the freshwater flux through Nares Strait over the 2007-2009 period relative to the 2003-2006 period, during which a stable arch did form at that location. We now add references to the recent studies of *Ryan et al., 2017* and *Munchow et al., 2016* in the text (section 5.1.3).

A direct comparison of the model simulations to the ice area or volume flux estimated for instance by *Kwok et al., 2010* is not trivial: it would require at least the knowledge of the temporal and spatial evolution of the wind forcing (and perhaps of ocean currents) as well as of the coverage and thickness of ice over Nares Strait over that time period. In the absence of these informations, we compared time series of the meridional component of the simulated ice drift velocity averaged across the constriction point between Kane Basin and Smith Sound for different ice cohesion scenarios to illustrate the impact of ice strength on the simulated outflow.

Especially, regarding the comparison to VP-models more caution is required. If no reference VP simulation is performed, the differences of the referred studies need to be considered, as most of the studies do not agree in resolution or region used in the experiments presented here. For example, it is not clear, if VP models at the same resolution would not lead to the same type of "ridging" behavior; as long as it is not clear, it shouldn't be claimed. The detailed overview which points need further discussion are listed below in the minor comments.

2 EVP studies were mentioned (*Dumont and Gratton, 2009* and *Rasmussen et al., 2010*), in the context of simulating ice bridges, not ridges, and the resolutions of their simulations are indeed comparable to the one used here, at least in the case of *Dumont et al., 2009*. As mentioned in the response to a later comment, the Maxwell-EB model produce similar results at lower resolutions. Moreover, as discussed below, we insist on the fact that no direct comparison was made here between the Maxwell-EB and VP/EVP model simulations.

Again, we insist on the fact that it was not claimed in the original version of the paper that the VP model couldn't reproduce the same type of "ridging". What was stated is that the Maxwell-EB model can reproduce characteristics of the observed ice thickness distribution with a very simple redistribution scheme. As mentioned in the response to your later comment, *rheologies* and *thickness redistribution schemes,* are *distinct* components of a sea ice model.

In Section 3, a sea-ice model using the Maxwell-EB rheology is outlined. However, due to the reference to individual equations in Dansereau, et al. (2015) the readability is reduced. A summary of the model equations cited from Dansereau et al. (2015) in an appendix would assist the reader. In addition, if you would include the simplifications that are made in your experiments into the model description, it is directly clear to the reader what equations are solved in your model. For example, the full momentum equations are given

in Equation (3); one can easily miss the fact that many terms are not solved for (p10, l.26-27). Here the reduced equations could replace Equation (3), since this is all that is used in the manuscript.

We completely agree on the need for more information about the equations and numerical scheme. We now include a detailed description of both in an Appendix. We further comment on the simplifications to the equations which are listed in the paper (p. 10, lines 26 to 29) here:

- *No thermodynamics coupling.* The present implementation of the Maxwell-EB rheology is not coupled to a thermodynamics model. The goal of these numerical experiments is to investigate its dynamical behaviour. Including thermodynamics processes would complicate this investigation. Simulations are analyzed over a short time-period (3 days maximum). This point is now made clearer in section 4. In terms of referring to previous VP (EVP) simulations at comparable resolution, this simplification is justified as thermodynamic processes were also neglected in the study of *Dumont et al., 2009*.

- *No Coriolis acceleration and ocean tilt terms.* As mentioned in section 4, second and third paragraphs, forcing conditions are made as simple as possible to facilitate the analysis of the dynamical behaviour of the model. Hence the ocean is at rest and the wind forcing is uniform over the channel in both the idealized and realistic cases. The Coriolis term is neglected with the intention to retain symmetry in the forcing conditions. Because the Coriolis acceleration plus sea surface tilt term is smaller in magnitude than the air and water drag and rheology terms (*Steele et al., 1997*) this is not expected to have a significant impact on the results. Note that *Dumont et al., 2009* also neglected the Coriolis acceleration in the idealized case. Also, in the realistic case, the ocean in *Dumont et al., 2009* is initially static.

- *No acceleration and advection term in the momentum equation.* Scaling analysis show that these terms are small (see *Dansereau et al., 2016*, section 4.1.4) Most sea ice models now include the $\partial \upsilon / \partial \tau$ term, however the advection term is still neglected in a number of, if not most, sea ice and ice-ocean coupled models (e.g., LIM3, FESIM/FESOM, ...). Additional simulations have shown that including both terms in the momentum equation does not affect the simulation results reported here. To correct the error made in the thickness redistribution scheme (see your later comment), all simulations needed to be run again for the resubmission of the paper. Both the acceleration and the advection terms are included in the corrected version of the model used to perform these simulations. The results are in all aspects very similar to the previously reported results such that all the conclusions of the paper remain unchanged.

We corrected the reported spelling and structure mistakes and respond to your minor comments below.

Minor comments:

page 1,l.7: "in" -> into page 1,l.8: "Strait" -> strait page 1,l.9: "different dynamical behaviours" -> various dynamical behaviour

page 2, l.26: "to" -> and?

page 2, l.32: "This rheological framework typically does not account for (uniaxial or biaxial) tensile strength." Not quite true, there is uniaxial tensile strength in Hibler elliptical yield curve (see your figure 2), but there's usually no isotropic/biaxial tensile strength; adding tensile strength is another option for the VP model and has been used to improve simulations of land fast ice (Lemieux et al 2016, Olason, 2016). Could be mentioned in this context, too.

As shown on figure 2, there is no uniaxial tensile strength in Hibler's elliptical yield curve. Instead, there is only *biaxial tensile-compressive strength* (accounted by the portion of the curve in the second and fourth quadrants, see stress state 2, Fig. 2). Uniaxial tensile strength is represented on Fig. 2 by stress state number 1 : $\sigma_1 < 0$ and $\sigma_2 = 0$ (or the inverse, by symmetry with the $\sigma_1 = \sigma_2$ axis). Biaxial tensile strength implies resistance of the material for $\sigma_1 < 0$ *and* $\sigma_2 < 0$. This state of stress is now represented schematically as stress state number 0 on figure 2.

Thank you for these references. We are aware that the elliptical yield curve has been modified especially in the context of modelling landfast ice, which has been related to the phenomenon of arching between islands. However, as both these papers are really concerned with the phenomenon of landfast ice, and landfast ice is also influenced by other phenomenon such as the grounding of keels, we beleive that including these reference at this point in the paper (in the discussion of the phenomenon of ice bridges, in particular in Nares Strait) would take the reader away the main point, which is that we are testing a new rheology on the basis of its capacity to represent the phenomenon of arching.

page 2, l. 30-32: Dumont also used EVP

Thanks for catching this. This is now corrected.

page 4, l. 13-14: That's a speculation. It would be nicer to actually show that this can work or not work with

VP models at high resolution (better than 4km grid spacing).

“It is not clear” is not speculation. We however remove the work “better” in this sentence, which indeed implies a comparison to VP models.

page 4, l.13: “. . .(e.g., see Fig. 1b and Sodhi,1977)” wrong citation

page 4, l.20: Section 2.2 “Ice ridges”. First paragraph is relevant for the following analysis of the ice thickness distribution. The last three paragraphs can be shortened or discarded, as neither a VP-model nor an ITD is used later on.

We do not agree with this comment: this discussion of ice thickness redistribution schemes is relevant and necessary to interpret the results discussed in this paper. We also stress the fact the thickness distribution scheme is a component *independent* of the rheological framework implemented in a sea ice model. VP rheology models actually use both the schemes described here. The only sentence that discusses the VP model in these paragraphs is the following:

“Nevertheless, it is still unclear to this day if, when incorporated in viscous-plastic type models, either of the two-level scheme or the multi-categories scheme, even when tuned, is able to reproduce the form of tail of the PDFs calculated from Arctic sea ice thickness measurements (e.g., *Flato et al., 1995)*.”

This statement refers to a 22 years-old paper. Other more recent VP or EVP model studies in which an ice *thickness distribution* was represented (otherwise only thickness fields are discussed) were not found, which adds to the fact that the point made in this sentence “is still unclear”. We however agree that the mention of VP models here is unnecessary since *the goal of the paper is to demonstrate that the Maxwell-EB model, with a very simple redistribution scheme, is capable of reproducing the exponential tail of the ice thickness distribution*. We therefore remove this sentence and add another one at the end of this section to stress this later point.

page 5, l.17: wrong citation, I guess Hibler, 1980 is meant? page 5, l.19: “badly” -> poorly

page 6, l. 5: “details” -> detail

page 6, l. 5: “recall” -> repeat?

We changed it for “review”, as suggested by reviewer 1.

page 6, l.15: This definition is unfortunate and unintuitive. 0 should be “undamaged” (zero damage) and 1 should “completely damaged” if d is called “damage”. d feels more like “integrity” for the material, but it’s just terminology . . .

Indeed, in solid mechanics conventions *d* in this case would have the meaning of “continuity”. However, this just terminology that helped us simplify the writing of some equations while developing the model. As this is the definition included in the paper describing the Maxwell-EB rheology (*Dansereau et al., 2016*) we do not modify it in the present paper.

page 6, l. 22: Isn’t Equation (2) t = 2C[(μ2 + 1)1/2 + μ] 1 ? At least in Danserau et al. (2016) Equation (7),(8) and (10) are not consistent and I guess the exponent -1 is missing in Equation (10).

You are right: thank you for catching this. This is also a mistake in *Dansereau et al., 2016*. Here we correct this mistake and write σ_t as it is implemented in the code, that is, $\sigma_t = -\sigma_\chi/q$, and add the definition of *q*, the slope of the damage criterion. We also add a footnote to report the mistake made in *Dansereau et al., 2016,* Eq. 10.

page 6, l. 27-28: Unclear, if it represents refreezing of leads, how can it be independent of pure thermodynamics, please explain/rewrite/elaborate

You are right, this formulation is confusing. Healing is “distinct” but not “independent” from thermodynamics processes, as obviously, in a coupled dynamic-thermodynamic model, the rate of healing should depend on the air and ocean temperature. In the present, uncoupled, implementation of the model, it is constant. What was meant is that, on a modelling point of view, healing is not the same process as thermodynamic growth because it allows the level of damage variable to increase *at most* to its undamaged value (*d* = 1) and does *not* allow the mean ice thickness nor the ice concentration to increase. The sentence is rephrased as “This mechanism is distinct from pure thermodynamic growth (...)” and the reader is referred to *Dansereau et al., 2016* for more precision about healing.

page 8, l.9: Equation (3); Please also state the simplified version of the equation that is actually used by the model, to prevent misunderstandings.

This is now incorporated in the Appendix, together with the description of the numerical scheme.

page 8, l. 21-22: Mechanical redistribution in your formulations is represented by the divergence term, see next comment about Equation (7).

We address this point in the response to your next comment, below.

page 9, l. 1, Equation (7) That is wrong: h is defined as the mean thickness (per grid cell area), so something like a volume of ice in the cell. The ice volume does not change if A > 1 is reset to A = 1, but the (mean) thickness of the thick ice hthick = h/A is increased, and there is no extra contribution to Sh. (See also Schulkes (1995), JGR.) This should be corrected in the text and also in the model, if the model actually implements this extra (spurious) tendency in Equation (4).

There was indeed an error made in the redistribution scheme, which lied in the fact that for A > 1, *h* could increase both through convergence and through the prescribed redistribution (equation 7). To correct this mistake, we have modified the parameterization as follow: the thickness of thick ice, $h_{thick} = h/A$, is advected passively with the flow for $A < 1$. This means that under convergent motion, there is no ridging if $A < 1$, but the ice volume (*h*) can effectively increase if the ice concentration over a grid cell increases. If $A > 1$over a given grid cell, it is reset to $A = 1$, and the mean ice thickness, *h*, (equal to the thickness of thick ice when $A = 1$) is increased (equation 7): hence the ice volume in that grid cell increases.

As mentioned in this section, in the present implementation of the model we seek to account for mechanical redistribution of the ice thickness in the simplest possible manner, so that to test the input of the rheological framework, i.e., its representation of ice deformation, on the thickness distribution. *In Schulkes et al., 1995* the divergence term is weighted as a function of *A.* In the ice concentration equation, this term is penalized as *A* increases from 0 to 1 and is zero for $A = 1$. Conversely, in the thickness equation, it is penalized as *A* decreases from 1 to 0 and is zero for $A = 0$. As opposed to the scheme presented in *Schulkes et al.,1995,* we do not suppose ice ridging occur for $A < 1$ and assume ice riding occurs only for $A \geq 1$. This avoids using any weighting/penalty function based on sea ice concentration, which would imply introducing additional parameters and which is not well constrained by observations.

Our approach is therefore simpler and more similar to that of *Hibler 1979*, in which the adjustment to the conservation equations for *A* and *h* occurs abruptly when and where $A = 1$, as discussed by *Schulkes, 1995*. For $A < 1$, our scheme is equivalent to that of Hibler 1979. For $A \geq 1$, the schemes differ. Our approach is as follow: the same differential equation is still solved for *A*, with a manual adjustment to $A = 1$, and ice thickness is adjusted for the excess ice concentration. This redistribution scheme conserves the ice volume and, compared to the Hibler 1979 scheme, has the advantage of not creating any spurious oscillations in the solution (which happened due to the abrupt change in the differential equations at $A = 1$, see *Schulkes et al., 1995*).

In correcting the paper, we have also addressed some issues with the presentation of the conservation equations for *A* and *h* and thickness redistribution scheme in the original version of the paper.

- First, the reference to *Hibler, 1979* for the parameterization of the ice thickness redistribution was a mistake. We drop the reference to *Lietaer et al., 2008* (who seem to have made the same error as we initially did with their redistribution scheme based on *h*). We also drop the reference to *Thompson et al., 1988*, as we suspect their redistribution scheme might also not be coherent.
- As pointed out by reviewer 1, there is a typo in equation (7), also found in the text (p. 8, line 27), and the right form, now corrected, reads:

  $h^{+} = \max[0, (A-1)]\, h$.

  This is the formulation used in the code, hence this typo does not impact the results reported here.
- As pointed out by reviewer 3, there was also a typo in the equation appearing on line 18, page 19. The correct expression, implemented in the model, is $\nabla \cdot (h u) = u \cdot \nabla h + h \nabla \cdot u$
- The adjustment on the excess ice concentration and associated redistribution of ice thickness (given by equation 7) is made a second numerical step, after solving the conservation equation for the ice thickness. This was discussed on page 13, lines 12 and 13, but this might not have been clear in the original version of the paper because the numerical scheme was not described in details. The inclusion of the appendix now clarifies this point.

The model simulations presented in the newly submitted version of the paper have all been corrected for this error in the thickness redistribution scheme. This correction has no significant effect on the reported results. The simulated ice thickness is somewhat lower than in the previous simulations, as expected from the removal of the extra growing tendency on *h*. However, both the PDF of the mean ice thickness in the idealized and realistic case show a similar shape and evolution, such that the main conclusions drawn form these numerical experiments remain unchanged.

page 9, l. 4-5, Equation (8) and (9): Just for clarity, a dependence on the thickness as in Hibler (1979) is not needed, as the internal stress is used in the momentum equation?

Yes, this is right. We now add a mention to this effect when introducing the form of the momentum equation solved in the simulations (A1) in the Appendix. The mechanical parameters (E, η, C) are intrinsic properties of the ice cover, as a material, and are independent of its thickness. For instance, *E* is an elastic modulus (Nm^{-2}), not a rigidity of the ice plate (Nm^{-1}). In particular, the fact that *C* is independent of ice thickness is important here as the contribution from thickness and cohesion to the strength of the ice cover are differentiated (in section 5.1.3).

page 9, l. 8: "widely" -> is widely page 9, l. 22: "(Kwok et al., 2010))" -> (Kwok et al., 2010)

page 10, l.11: "northerly, wind stress" -> not sure about this: northerly winds, but the stress is acting towards the south, should be made clearer I think.

This sentence and the next are rephrased as: "Consistent with observations of orographic channelling, an along-channel, i.e., southward, wind stress, τ_a, is applied. The stress is spatially uniform and increased steadily between (...)".

page 10, l.24: "transport of the cohesion, C" there are no sources and sinks of cohesion? How realistic is that? Please comment and elaborate the cohesion equation in Dansereau et al. (2016), this is not discussed.

There is no sources or sinks of cohesion in the model. The field of cohesion is set at $t = 0$, i.e., as other initial conditions, and is advected passively with the flow. As discussed on page 12, lines 3 to 12, for ice entering the channel through open boundaries, the cohesion is set over each model element as it is set for the initial conditions, that is, by drawing a value randomly from a given uniform distribution.

As mentioned in our response to your previous comment, the cohesion is an intrinsic property of the material which sets its mechanical strength (its resistance to pure shear). Here *C* is a grid-cell averaged quantity and is allowed to vary locally to represent the natural homogeneity/heterogeneity of the material (various defects of different scales, for instance brine pockets at the small scale or the presence of different types of ice, e.g, a mixture of first year and older floes, smaller and larger floes, etc., at large scales). A comment to this effect in now added at the end line 24, p. 10. The noise introduced on *C* could alternatively be applied to another mechanical parameter, for instance, the elastic modulus (*Amitrano et al., 1999 and others*).

As this property is independent of ice thickness, there is no source of *C* due to ice thinning/thickening. In progressive damage models, cohesion could be made to depend on the level of damage. Simulations have shown that this causes an even more extreme localization of the deformation and damage in a material (*Lucas Girard*, *Ph.D. thesis*). We cannot think of other sources or sinks of cohesion.

The cohesion equation (a transport equation) is now included in the appendix and referred to in the text.

page 10, l.32: What is the reference for the Young's modulus? Same as for the Poisson's ratio?

The value of the Young's modulus (0.585 GPa here) is of the same order of that used by *Girard et al., 2011* (0.35 GPa) and implies with an elastic shear wave speed of 500 ms^{-1}, consistent with that reported by *Marsan et al., 2011* (440 ms^{-1}). These references have been added in the text. Estimates of the Young's modulus are highly variables. The value used here is close to the lower bound of the range of reported value. Using a higher value (2.34 GPa), consistent with a shear wave speed of 1000 ms^{-1} and on the order of in-situ seismic measurements as reported by *Timco and Weeks, 2010* (between 1.7 and 9.1 GPa, with higher values for low brine volumes, i.e., fresher ice) however does not change the mechanical behaviour of the model. This has been verified in the context of the present channel flow simulations. As mentioned in our response to your later comment, a higher value of E_0 allows stable ice bridges to form in the channel for somewhat lower values of cohesion than the ones reported here. The exact values of E_0 and *C* to employ in the model at a given spatial resolution are therefore not strictly constrained.

page 11, l.5-8: The physical role of healing is unclear and needs to be explained better. It is clearly connected to the thermodynamics (in contrast to earlier statements in the manuscript) . . . Please elaborate . . .

Healing is linked to the level of damage of the ice cover, *d*, which represents the density of cracks/leads within a model grid cell and the impact of these features on the sea ice rheology. In the present model, this variable is independent of the ice concentration, *A*. Healing represents the refreezing within these cracks/leads and allows a damaged ice cover to recover at most its undamaged mechanical strength. As explained in the response to your earlier comment, healing is theoretically not independent from thermodynamics, as the rate of healing should depend on the difference in temperature between the atmosphere above and that of the ocean below. In the present model, healing is not coupled to a thermodynamics component and the healing rate is constant in both space and time.

Because of the absence of thermodynamic-dynamic coupling in the present model, *d* can increase locally due to healing, but the ice concentration, *A*, is not allowed to re-increase by the same process. Where the ice cover is highly fragmented but dense (high concentration), allowing the ice to heal without re-increasing the ice concentration is physically sound. However, where the ice concentration drops such that mechanical interactions (i.e., the rheology term) becomes insignificant, this absence of thermodynamic-dynamic coupling leads to a situation where *d* can re-increase up to its undamaged value (1), but *A* can drop to 0, representing open water.

To deal with this unphysical situation, in the present simulations we impose a cutoff on healing when and where *A* < 0.75, which essentially occurs when the ice detaches from a bridge or a coast. As when *A* < 0.75, the rheology term in the momentum equation becomes negligible and the ice is in a free drift state, no matter the value of *d*, we find that imposing this cutoff and its specific value of *A* has no significant impact on the simulated dynamics.

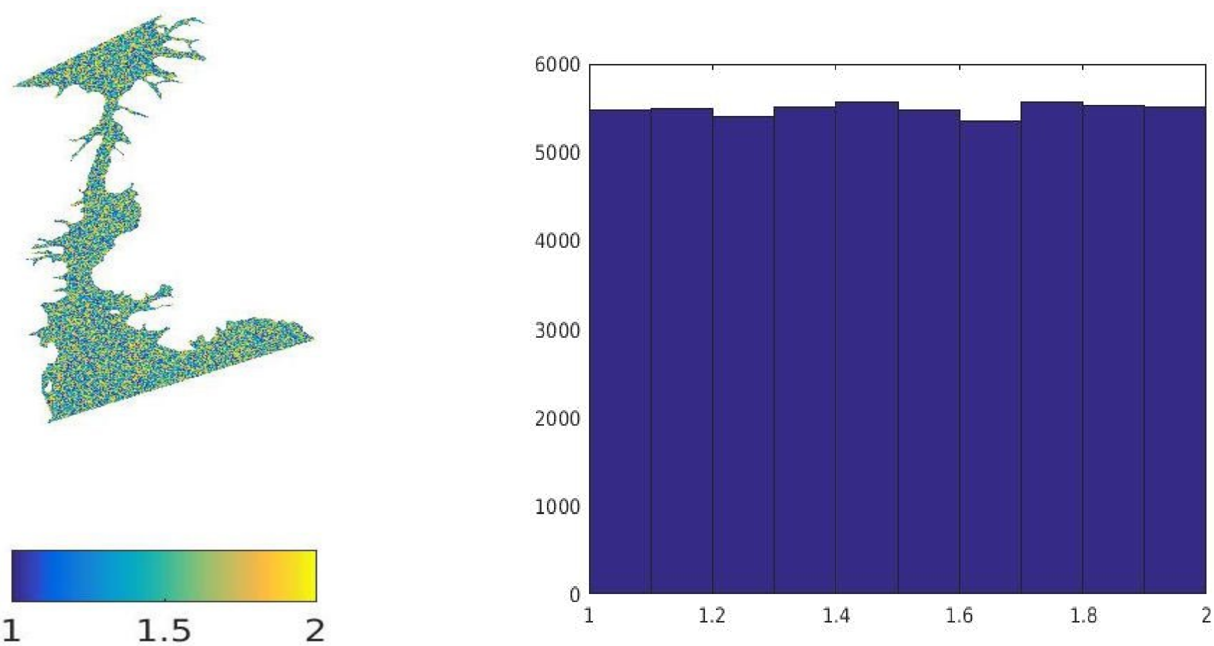
This point is now clarified on page 11.

page 12, l.7: “location of ice bridges is not prescribed” only through the random field of cohesion (the spatial pattern should be shown somewhere). I would like to see simulations with uniform cohesion; the model geometry should be irregular enough to make the model develop ice bridges, etc.

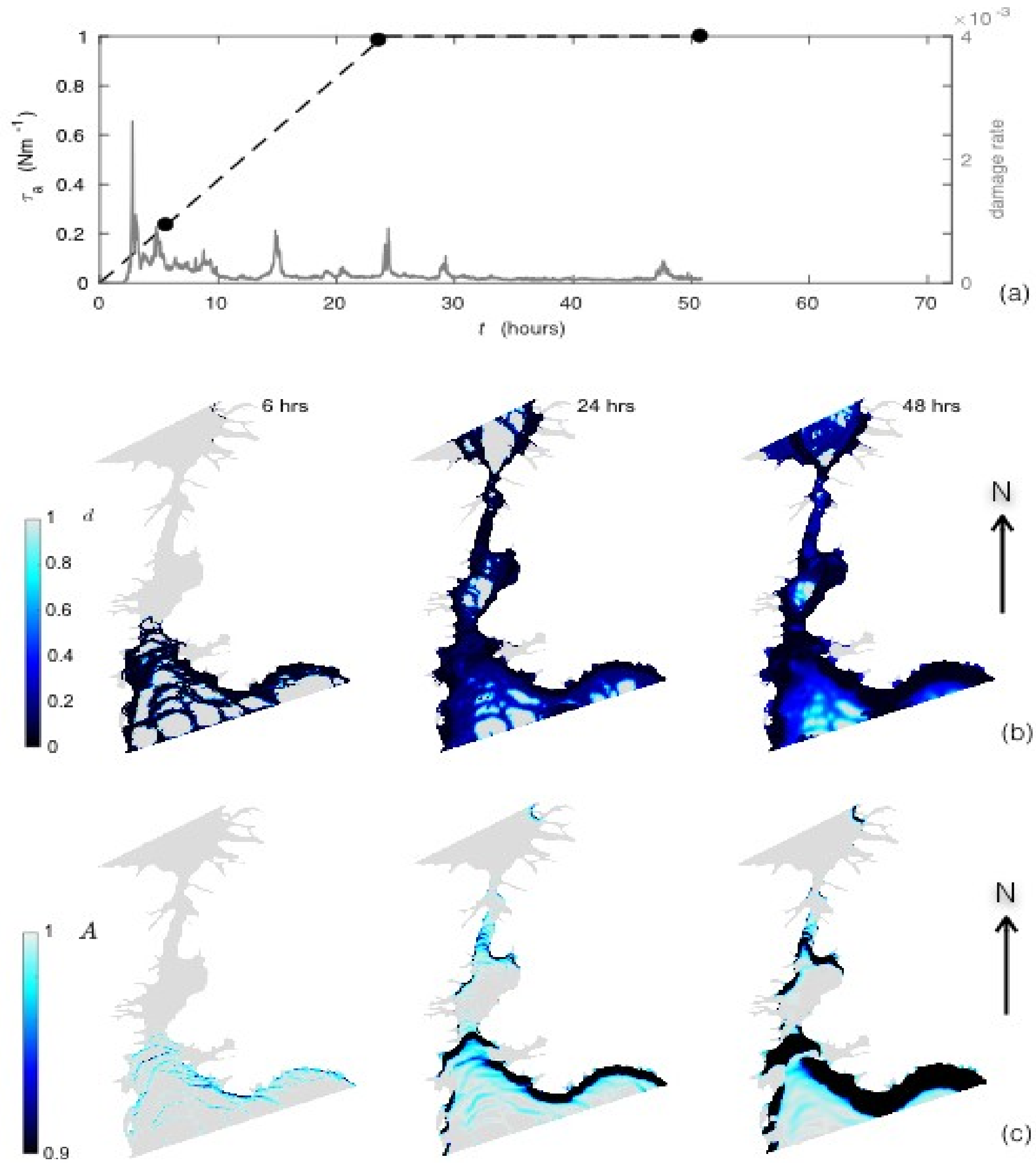
We do not agree with this comment. As explained in the text, the field of cohesion is *random*, hence, by definition, there are no spatial correlations introduced by the field of *C* in the model. Cohesion therefore does not prescribe the location of ice leads and bridges, only the mechanical behaviour of the model and the domain geometry does. A sentence is added to stress this point in the last paragraph of page 11. Simulations, both idealized and realistic, started from different fields of *C*, set as described on page 11, line 10 to page 12, line 2, have indeed been performed, and have reproduced the same location of the ice bridge.

The disorder introduced in the field of cohesion causes the *progressive failure* of the ice cover, *even under homogeneous forcing conditions* (see *Dansereau et al., 2016*). A sentence is added to clarify this point on page 6, after line 25.

These two points can be demonstrated by comparing the propagation of damage in a simulation in which noise is initially introduced in the field of cohesion (see figure 6) and a simulation started with a uniform field of *C* (see below). Highly damaged features emerge in both cases in similar locations. In both cases also, ice bridges develop in the same locations, which is therefore not attributable to a pattern in the field of cohesion but to the flow conditions and domain geometry. A notable difference between the simulation is the width of the first damaged features simulated by the model, that is, the features formed in initially undamaged ice (field b, *t* = 6 hrs). In the uniform cohesion case, these features are wider, due to the fact that all model elements can become over-critical and trivially fail, at the same time. This is also visible in the field of ice concentration (c) and translates into higher value of the damage rate (a) compared to the noisy cohesion case (see figure 6a). However, as discussed in *Dansereau et al., 2016* (see section 6.1), as soon as there are some damage present in the ice cover, the heterogeneities introduced in the stress field by these damaged features contribute and, over time, prevail over the noise in *C* in setting the location and timing of subsequent events. This explains why damage in an non-intact ice cover becomes highly localized even in the uniform cohesion case (*t* = 24 hrs, 48 hrs). In the present simulation, a highly homogeneous wind forcing is used and simulations are started from uniform ice conditions. If simulations were started from realistic, heterogeneous ice conditions, with non-uniform thickness and concentration, and used realistic, time and space-dependant wind forcing, the first damage events would probably be highly localized, independently of the degree of disorder introduced through the cohesion field (eg., *Bouillon and Rampal*, 2015).



Left panel: noise on the field of cohesion. This field is multiplied by C_{min}, such that $C \subset [C_{min}, 2 \times C_{min}]$. Right panel: distribution of the noise on the field of cohesion shown in the right panel.



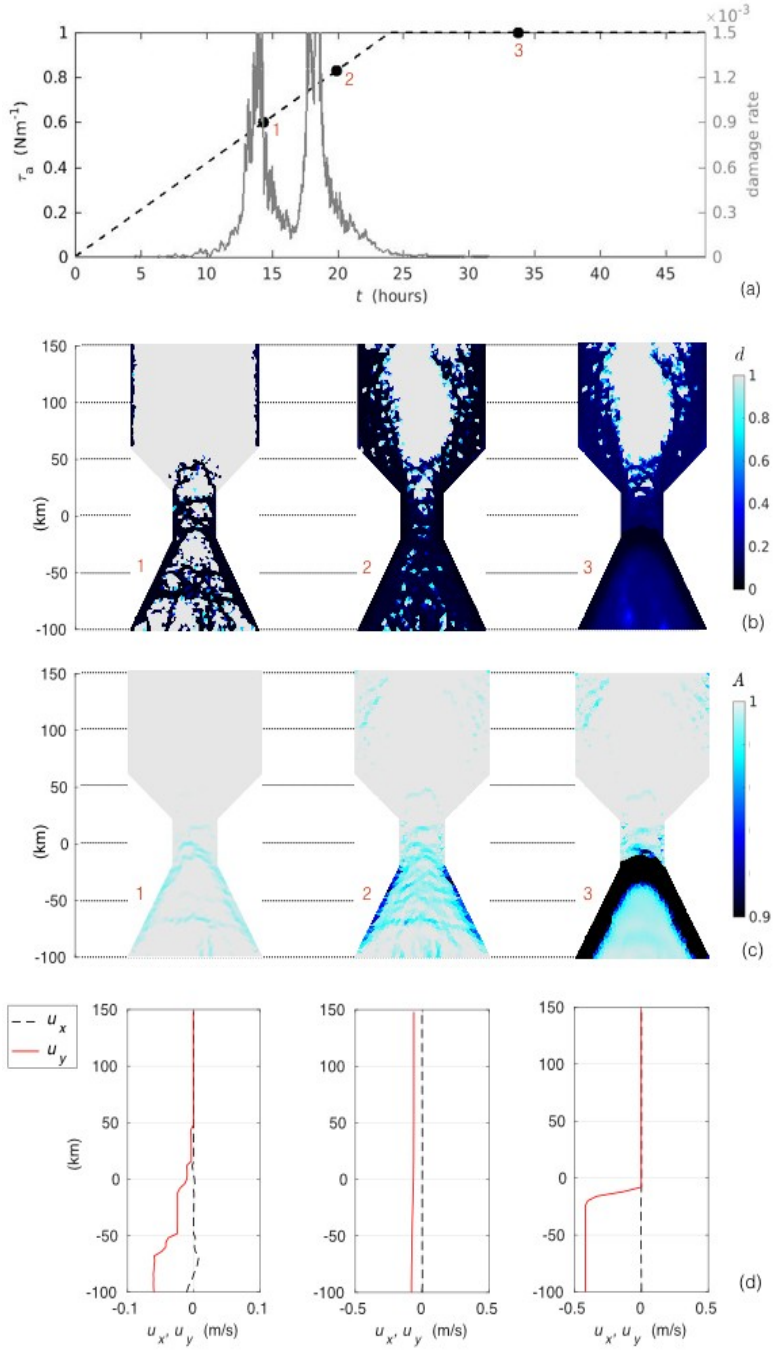
(a) Time series of the wind forcing (dashed curve) and of the damage rate (solid grey curve) over the realistic Nares Strait in a simulation using a uniform field of cohesion, C. Instantaneous fields of the simulated (b) level of damage and (c) ice concentration at t = 6, 24 and 48 hours.This simulations was run for about 50 hours instead of 72 hours as in figure 6 of the paper.

As the setting of the noise in the field of cohesion in both the idealized and realistic cases is described in the text (page 11, line 10-17), so that the reader can reproduce the results, and as a figure does not provide more information, we do not believe that including a figure of the field of *C* in the paper is necessary. An example of the random noise on the cohesion field and distribution of this noise is shown above for the realistic case.

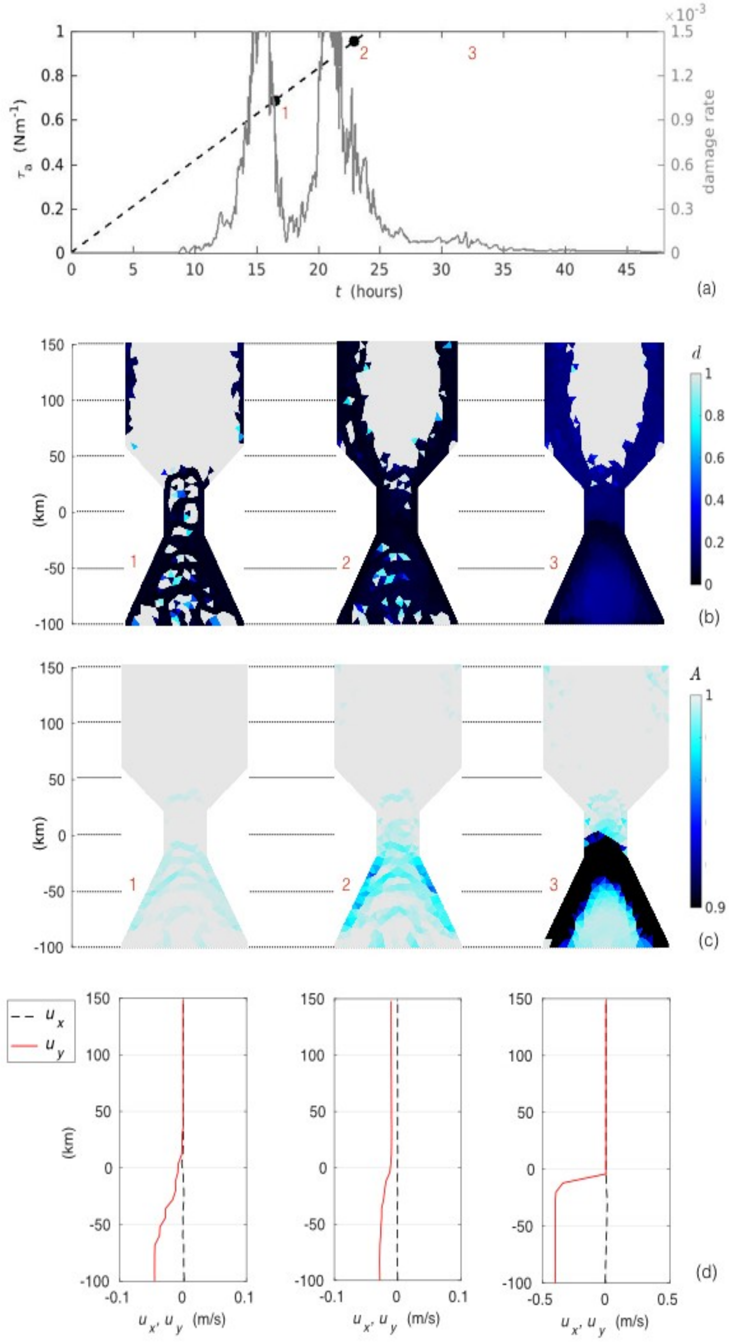
page 12, l.16 (and elsewhere): "(see (Dansereau et al., 2016))" -> (see Dansereau et al., 2016).

page 12, l.18: For the claims made in the introduction and background sections, VP simulations at this resolution are absolutely required (have not been done to my knowledge). Please tone done the statements in the appropriate places.

The EVP simulations of *Dumont et Gratton, 2009* have a resolution of approximately 3 by 4 km, which is comparable to the resolution used here in our realistic experiments (we note that there is an error on the horizontal resolution, i.e., an inversion between the latitude and longitude, reported in their table 1, otherwise their model resolution is something like 17 km by 1 km). The authors state that there are 14 grid cells across the narrowest point (46 km) of their channel in the idealized experiment which corresponds to the narrowest point between Kane Basin and Smith Sound. In the present realistic and idealized experiments, there are about 19-22 grid cells at the narrowest point between Kane Basin and Smith Sound (56 km on our grid) where the main ice bridge form, which is again comparable. The other EVP simulation of ice bridges in Nares Strait mentioned here (*Rasmussen et al.*, 2010) indeed use a coarser resolution (between 4 km in the Lincoln sea, 83 N, and 10 km in Baffin Bay, < 74 N, and about 7 km between Kane Basin and Smith Sound). However, as mentioned in section 5.2 about the ice thickness distribution in the idealized case, lower resolution (4 km, which gives 13-14 grid cells across the constriction point of the idealized channel, as in *Dumont et al., 2009*, and 8 km, which gives 6-7 grid cells across the constriction point, as in *Losch and Danilov, 2012*) idealized simulations produced similar results. It is also the case for other variables (level of damage, ice concentration, velocity profiles, etc., see figures below) and for the realistic experiments at lower resolution (not shown) which demonstrate that the results obtained here do not depend on the model resolution.



(a) Time series of the wind forcing (dashed curve) and of the damage rate (solid grey curve) in an idealized channel simulation using C_{min} = 20 kPa. Instantaneous spatial distribution of (b) the level of damage and (c) ice concentration at the times indicated by the numbers 1, 2 and 3 on the time series of panel (a). Instantaneous profiles of the vertical and horizontal velocities at the times indicated by the numbers 1, 2 and 3 on panel (a). The horizontal resolution is of 4 km.



(a) Time series of the wind forcing (dashed curve) and of the damage rate (solid grey curve) in an idealized channel simulation using C_{min} = 20 kPa. Instantaneous spatial distribution of (b) the level of damage and (c) ice concentration at the times indicated by the numbers 1, 2 and 3 on the time series of panel (a). Instantaneous profiles of the vertical and horizontal velocities at the times indicated by the numbers 1, 2 and 3 on panel (a). The horizontal resolution is of 8 km.

Modifications have been made in the text regarding the comparison between the Maxwell-EB and the VP/EVP rheology (see our responses to your earlier comments). We stress the point that this paper was never about making a comparison between the two types of models, *but to demonstrate the capabilities of the Maxwell-EB model.* Moreover, we believe that the capability of a model to represent a given physical phenomenon should *not* depend on model resolution, as long as the resolution is sufficient to resolve the relevant processes.

page 13, l.1-2: please state the range of x

page 13, l.16-17: "(see Dansereau et al. (2016))" -> (see Dansereau et al., 2016)

page 13, l.19-20: I think this statement requires, that you have tried a fully implicit scheme and compared the results. Have you? If not, this statement is no really supported by anything and should be changed.

Yes, we have tried a fully implicit scheme, in which all variables were updated as part of the fixed point iteration. This did not have significant impact on the simulation results both in highly idealized and realistic cases, as mentioned in the text.

page 14, l.8-9: Please say, how much the "drift velocity on the order of that associated with strictly elastic deformations within an undamaged ice cover." really is (in m/s or cm/s or whatever) so that others can compare.

This reference has been added (***u*** is on the order of 10^{-5} ms^{-1} maximum for strictly elastic deformations).

page 14, l.11: "relatively undamaged" -> rephrase to "stagnant ice with low damage" or similar

Ok.

page 14, l. 20-21: "the width of the distribution of C impacts the rate of propagation of the damage, with the propagation being more progressive for a larger distribution." Since the cohesion appears to be an important parameter, it would be useful to add more information about the choice of C, i.e. the actual distribution of C that is generated (page 11, ll.10) in case the reader would like to reproduce the results.

Because this comment is not relevant to understand the results presented here, it is now removed. The main point of using different values of cohesion in mentioned in the previous sentence, which is that the minimum value of cohesion over the domain controls the timing of the onset of damaging in the simulations.

Idealized simulations exploring the specific role of disorder (i.e., the width of the distribution of *C* here) in elasto-brittle models are now being performed, and show that this statement, "the propagation being more progressive for a larger distribution" is not exactly correct. We therefore believe that removing this sentence will avoid any confusion on this point. Besides, c hannel flow simulations with a uniform cohesion have produced results similar to that reported here (see our response to your earlier comment), demonstrating that the width of the distribution of cohesion is not an important factor in these simulations.

The distributions of *C* that are generated for these simulation are explained on page 11, line 10-17 (see our response to your earlier comment).

page 15, l.2: "differs" -> differ

page 15, l.23: "(see Fig. 4b and 4c, panel 3)" Should be Fig. 5b and 5c. . .

page 15, l.23-26: "This is an important point, as standard viscous-plastic sea ice models do not account for pure uniaxial or biaxial tensile strength and hence would not be able to reproduce the formation of a stable ice arch with self-obstruction to flow under the conditions simulated here." I don't agree: (1) From the figure, the location of the arch is not visible if you mean it is defined by the location of black elements. (2) the de- tails of the yield curve (Figure 2) should not matter, one can tune the elliptic yield curve to resemble the Mohr-Coulomb and tensile failure criteria (see Figure 1 in Lemieux et al, 2016). (3) even without isotropic/biaxial tensile strength, Dumont could simulate arches with VP rheology, so do Losch and Danilov (2012) in similar idealized simulations, even with "a standard VP model" for order 1000 days. (4) why do VP models not account for pure uniaxial tensile strength? I think that this statement needs to change.

(1) The location of the ice bridge is not defined by the location of black elements. In the text, the location of the ice bridge is associated to *the collocation of a minimum/maximum in the second and first principal stresses*. The location of the ice arch is clearly visible from the profile of ice velocity and (now included) the field of ice concentration. This last point is now mentioned in the text. Also, in the following sentence, there was a mistake : "downstream" should be "upstream".

(2) First, it is important to stress the point that the yield/damage criterion and the rheology (i.e., the constitutive law) are separate components of a mechanical model. The details of the yield curve do matter because to sustain ice bridges, the ice needs to have some cohesive strength (see *Dumont et al., 2009* and *Lemieux et al., 2016*). *Lemieux et al., 2016* refers to the standard elliptical yield curve as accounting uniaxial tensile strength (2^{nd} page, 3^{rd} paragraph). This wording is false. The standard elliptical yield curve accounts for some biaxial tensile-compressive strength (see our response to your earlier comment), uniaxial

compressive strength but no uniaxial tensile strength. In this paper, the authors have modified the standard elliptical yield curve to account for uniaxial and biaxial tensile strength for a better representation of landfast ice in VP models, hence implying that the details of the yield curve do matter.

(3) As mentioned in our response to your earlier comment, the elliptical yield curve used by *Dumont et al., 2009* and *Losch and Danilov, 2012*, does not include biaxial (or uniaxial) tensile strength, but biaxial compressive-tensile strength and uniaxial compressive strength. Therefore ice in these models can not sustain biaxial tensile stresses. Here, as shown by the profile of the principal stress components, the state of stress just upstream of the ice bridge is *biaxial* tensile, which demonstrates that the bridge sustains biaxial tensile stresses. In the paper, we thus make the point that models that do not account for biaxial tensile strength would not be able to reproduce a stable ice bridge in the conditions simulated here, i.e., in which the states of stress are biaxial tensile.

(4) We do not understand this question fully because of your earlier comment, which states that there is uniaxial tensile strength in the standard elliptical yield curve. There is indeed no uniaxial nor biaxial tensile strength in the standard, Hibler elliptical yield curve. This yield curve was chosen based on the early AIDJEX assumptions that sea ice did not exhibit pure tensile strength (see *Coon et al., 2007*).

We made some adjustment to this paragraph (and figures) to indicate the location of the ice bridge as well as the states of stresses upstream of this bridge more clearly. We also made modifications to section 2.1 and figure 2 to better explain what is cohesion and the difference between uniaxial/biaxial tensile, biaxial tensile-compressive and uniaxial compressive strength. We also modified the statement concerned by this comment as "This is an important point, *as models based on the standard elliptical yield curve* do not account for uniaxial or biaxial tensile strength and hence would not be able to reproduce the formation of a stable ice arch with self-obstruction to flow under the *stress* conditions simulated here" and believe that otherwise it does not need to change.

page 16, l.24: In this comparison (Figure 8), one might ask why the specific failure curves where chosen differently for the model, when there are estimates for the parameters available (c = 250, and μ = 0.9). Should be discussed somewhere.

This value of *q* (i.e., μ) and σ_c was taken by *Weiss et al., 2007* and *Weiss and Schulson, 2009* to draw the Mohr-Coulomb envelope on this figure because it was the one available value, reported by *Schulson et al. 2006a* for the failure envelope of first-year arctic sea ice obtained from biaxial tests in the laboratory at −10 °C. This is now mentioned in a footnote. In the Maxwell-EB model, we use μ = 0.7, equivalent to an internal friction angle of 35 degrees, a value commonly used for geomaterials and ice (*Byerlee, 1978* and *Jaeger and Cook, 1979*). A lower value of *q* could also be deduced from figure 8a. Conversely, using μ = 0.9 (internal friction coefficient of 42 degrees, not shown) does not impact the behaviour of the Maxwell-EB model.

page 17, l.3: "later" -> more recent ?

We changed it for the 1990's, which is the correct period reported by *Barber et al., 2001*.

page 17, l.16: "According with"-> In line with

We changed it for "in accordance with", as suggested by reviewer 1.

page 17, l.21: "'differentiated" does not sound right, rephrase if necessary

We now use "distinguished".

page 17, l.29-31: "However, in all of the weaker ice cover scenarios (2002-2008 period and/or summer), none of the ice arches formed near the exit of Kane Basin nor secondary arches formed elsewhere sustain the applied wind forcing and all ice bridges eventually collapse." Is there a similar behaviour in observations in this period? Please add a comment.

Yes, a similar behaviour was observed over the same time period, as discussed at the beginning of section 5.1.3 (first paragraph). For instance, no ice bridge formed between Kane Basin and Smith Sound in the winters of 2007/2008 to 2009/2010, except for a 2 months period (*Munchow et al., 2016*). We have modified this paragraph to include this and a more recent reference (*Ryan et al., 2017*).

Since we perform simulations with an initially uniform ice thickness and simplified wind forcing, i.e., not representative of specific conditions over the period 1979-2001 or 2002 and 2008, we do not think making a direct comparison to ice conditions in the Strait during that period is relevant at this point in the text.

page 18, l.7: "widely different dynamical behaviours" -> a wide range of dynamical behaviour

page 18, l.7-9: The big question remains: how do you determine the appropriate cohesion? It appears to be vital parameter, similar to P* in Hibler's VP model.

Cohesion is indeed an important parameter in the model as it controls the shear strength of the ice and as for $C = 0$, the model would not allow any form of tensile strength. However, we do not believe a direct comparison to P^* is relevant. Indeed, in-situ stress measurements do indicate the importance of the cohesion parameter, by the fact that these measurements fit well a Mohr-Coulomb criterion with non-zero cohesion (see figure 8a). On the contrary, these measurements do not support the role of P^*, the biaxial compressive strength, as being a relevant parameter to describe the shape of the damage criterion (or yield criterion in the case of the VP model). The measurements do not give an indication of an appropriate value for this parameter either.

As mentioned on page 11, line 17, some studies (e.g., S*chulson, 2004; Weiss et al., 2007*) assume a scale effect on shear strength, set by the size of the defects (thermal cracks, brine pockets, ...) present in the ice cover. According to this scaling, lower values of *C* are consistent with larger defect sizes and a lower shear strength. It is difficult to infer a proper spatial scale for the in-situ stress measurements reported here (from Weiss et al., 2007), but it should be smaller than the spatial resolution of the present experiments, hence a lower cohesion should be used in the model.

As mentioned in section 4, the *highest* values of *C* employed here (i.e., the upper bound of the distribution of C in the case of C_{min} = 30 kPa, which is 60 kPa) are consistent with the in-situ stress measurements reported by *Weiss et al., 2007* (see figure 8b). We obtained the formation of stable ice bridges in the model for lower values of *C*.

However, the fact that we obtained the formation of a stable ice bridge in the present idealized and realistic simulations using C_{min} = 20 kPa does not mean this is the appropriate value of cohesion for sea ice or for the Maxwell-EB model, nor that it is the only value for with the Maxwell-EB model can reproduce the formation of a stable ice bridge between Kane Basin and Smith Sound. As mentioned in lines 7 to 9, this result depends on

- the prescribed initial thickness. Bridges form at lower cohesion for thicker ice. Here we used h_0 = 1 m but a higher h_0 might be more representative of ice conditions for some years.
- on the specific value used for the Young's modulus. A higher value allows the formation of stable ice bridges for lower values of cohesion. As mentioned in the response to your earlier comment, the value used for E_0 is at the lowest bound of the range of reported values.
- the magnitude of the applied wind forcing. In the model simulations, we increase the wind forcing up to 1 Nm^{-2} and hold it constant, which corresponds to a wind speed of 82 km h^{-1} or 22 ms^{-1}. While daily-averaged model wind stress values of 0.7–1.0 N m^{-2} have been reported in Nares Strait, see *Samelson et al., 2006*, a uniform, sustained wind stress of 1 Nm^{-2} for *several days* is most probably an overestimation of the reality. Were we made this choice of wind forcing to simplify the analysis.

Therefore, If we were to increase $h_{0,}$ increase E_0 and decrease the applied wind forcing, stable ice bridge would be obtained for lower values of *C*, and conversely for a lower h_0 and E_0 and higher wind forcing. In the passage you are reporting, we therefore made it clear that the goal of these experiments was not to determine an appropriate range of value for the cohesion.

page 19, l.5-6: "A Lagrangian model would perhaps be more suitable to simulate the edge of the detached ice"; or a better advection scheme with less numerical diffusion (i.e. higher order basis functions in your finite element method)

The diffusivity of the numerical scheme and order of the polynomial approximations used are described in the sentences above, from p. 18, line 31, to p. 19, line 3. The sentence you are referring to does not refer to diffusion, but to the fact that Lagrangian approaches, i.e., which follow ice particles, are better suited to track the ice edge. The use of higher polynomial approximation does not *change* the numerical scheme.

Also, we have replaced "more suitable" by "a more natural approach".

page 19, l.13-14: "Nevertheless, at all times the simulated probability density function is strongly asymmetric, consistent with thickness distributions estimated for sea ice with little history of melting (e.g., Haas, 2009)." Please discuss in how far this special experimental geometry with many coastlines and the low Cmin is suitable to compare to observations made for open ocean Arctic sea ice as described in Haas (2009).

Here, we referred to measurements from the open Arctic ocean with little history of melting specifically because the model does not represent thermodynamic effects and hence the simulated ice thickness distribution and hence a comparison with measurements from a region where the melting signal is important should not be made. Asymmetric thickness distribution have not been obtained from open Arctic ocean measurements only. For instance, *Hass et al., 2006* report an asymmetric thickness distribution with an exponential tail from AEM measurements at the entrance of Nares Strait. We now include this reference in the text.

Concerning the value of cohesion, a higher value (e.g., C_{min} = 20 kPa) also give a strongly asymmetric thickness distribution, however, it does not allow the thickness to increase to values as high as in the C_{min} = 10 kPa case in the same simulation time, only because ice bridges form and stop the flow of ice through the channel, hence reducing the amount of ice entering the channel that can be incorporated into ice ridges. This point is now clarified in this section. Moreover, as discussed in the response to your earlier comment, there is no observational nor physical evidences at this point to characterize C_{min} = 10 kPa as a "low" or "too low" cohesion for the ice cover.

page 19, l.18: This term (7) is not correct and should not be used. See e.g. Schulkes (1995), JGR, for correct equations and a nice explanation of ridging in general.

This is a typo in the text on the development of the term $\nabla\cdot(h\,u)$ (see response to reviewer 3 and to your earlier comment). This was not an error in the code.

page 19, l. 23: "Fig. 11b" -> Fig. 10b 10b

page 20, l. 2-3: "In coupled thermodynamic and dynamic models, a high density of leads is expected to impact the simulated heat fluxes between the atmosphere, the ice and the ocean (Smith et al., 1990)." This is not really a conclusion, but part of a discussion.

We agree and move this comment to the discussion part of this section (page 20, end of second paragraph).

page 20, l. 11-13: "the presence of land fast ice along. . ." This has hardly been discussed and comes as a surprise. Needs more attention in Section 5 if you want to keep this conclusion

We do not agree with this comment, as the presence of landfast ice is discussed in section 5.1.2 and 5.1.2, along with other features reproduced by the model. This remains in the list of conclusions. We have added additional references on the observed presence of landfast ice in Nares Strait.

page 20, l. 24: "a process that is known to be underestimated in VP models using a two-level scheme" This is new to me. At correspondingly high resolution I would expect a VP model to behave in a similar manner, see also Losch and Danilov (2012), Fig6. which shows very similar ice thickness distribution in a similar channel experiment.

The statement made here compares the thickening of the ice cover between a VP model with a two-level versus a multi-categogies thickness redistribution scheme. It is our understanding that in *Losch and Danilov, 2012*, a two-level categories scheme was used as was not compared to a multi-categories scheme. An ice thickness distribution was not computed in this study. The results reported represent a steady state after 10 years of integration and hence would not be directly comparable with the present Maxwell-EB simulations.

This sentence was moved to the discussion of the two-level and multi-categories scheme, section 2.2.

page 20, l.26-28: See above, I don't think, that you can say this, because you'd have to show that the same model configuration with a VP model would not have your thickness distribution. I am pretty sure that you would get a similar result.

The sentences you are referring to is:

"In the Maxwell-EB model, this capability of accounting for a sufficient thickening of the ice as well as the spatial localization of extreme thickness values arises from the appropriate description of extreme strain localization. On a mechanical point of view, this may therefore question the relevance of using multi-categories redistribution schemes."

The sentences therefore discusses the capability of the Maxwell-EB model, not the VP model, to represent the localization of increased ice thickness, in relation with the localization of ice deformation. The next sentence questions the use of a multi-categories thickness model versus a simpler thickness redistribution model to obtain this localization of high thickness values. As mentioned an earlier comment, the thickness redistribution scheme is independent of the rheology used and here, the VP model is not mentioned. Therefore this does not prevents us from writing this sentence.

page 21, l. 14: "later" -> recent

page 21, l. 33: "Haas, C.: Dynamics Versus Thermodynamics: The Sea Ice Thickness Distribution, p. 638, Wiley-Blackwell, 2009." Please correct citation as book chapter in Sea Ice (eds D. N. Thomas and G. S. Dieckmann)

page 22, l.10: “III, W. D. H.: Modeling a Variable Thickness Sea Ice Cover,. . .” -> wrong name

page 25, l.27: “Weiss, J. and Dansereau, V.: Linking scales in sea ice mechanics, Philosophical Transactions A, pp. –, doi:10.1098/XXXX, 2016.” Is this a submitted manuscript? If so it is not properly cited.

page 26, Figure 5: What is Cmin in this simulations? Did you consider to show a sea ice concentration plot for the idealized experiments as well? That would help to see the arches directly.

C_{min} = 20 kPa (it is the same simulation as in Figure 4, as mentioned in the caption). This is now also stated at the beginning of section 5.1.1. The corresponding fields of ice concentration are now added to this figure.

page 29, Figure 8: An indication of the probability of single stress states using a colormap or transparency would be helpful, to get an impression how frequently biaxial tensile states (and all other stress states) occur.

To indicate the proportion of each types of stress through time, a time series of stress state types (tensile, biaxial tensile-compressive, biaxial compressive) during the corresponding simulation is now included in Figure 8. Figure 8b (now 8c) corresponds to a snapshot at t = 72 hours, when the probability of each stress states and repartition in the principal stresses plane has stabilized. This point is now clarified in the text.

page 31, Figure 10: Why are the PDFs for x = 4km and 8km given at t=5days, whereas the other results are shown for t=3days?

Thank you for catching this. This is a typo from an earlier simulation. The PDFs for x = 4 km and 8 km are indeed given for t = 3 days. This has been corrected in the figure caption.